THE

MATHEMATICAL GAZETTE.

EDITED BY

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WITH THE CO-OPERATION OF

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THE MATHEMATICAL ASSOCIATION.

THE Annual Meeting of the Mathematical Association was held in the Medical Library of King's College, London, on Saturday, 28th January, 1905. An exhibition of apparatus for use in the teaching of mathematics was held in the adjoining room by MR. W. J. BROOKS, of 33 Fitzroy Street, W.; MR. CUSSONS, of Broughton, Manchester; and the Educational Supply Association. About sixty members and visitors were present.

The PRESIDENT (PROFESSOR A. R. FORSYTH) took the chair at 3 o'clock, and called upon one of the Honorary Secretaries to read the Report of the Council.

MR. C. PENDLEBURY read the report as follows:

The Council have to report that no General Meeting was held in May or October, no papers having been offered.

Since the last Annual Meeting 30 new members have been elected, 6 have died, and 18 have retired, resulting in a small net gain of 6 members.

The Association now consists of 402 members, of whom 10 are honorary, 19 life members by composition, 47 life members under the rule which released those who had paid 10 annual subscriptions from all further liability, 30 who were formerly life members under this rule, but who voluntarily resumed payment of the annual subscription, and 296 ordinary members. Briefly, of the 402 members, 19 are life members by composition, and 326 are liable for the payment of the annual subscription.

The six members who have died during the year are:

Professor G. J. Allman, F.R.S., formerly Professor of Mathematics at Queen's College, Belfast.

Dr. R. Biggs, Head Master of the Royal School, Portora, Enniskillen, who was drowned on June 23rd, 1904.

R. W. H. T. Hudson, Fellow of St. John's College, Cambridge, and Lecturer in Mathematics at Liverpool University, who was killed on 20th September, 1904.

The Venerable Archdeacon Hughes-Games, Vicar of Hull, who was an original member of the A.I.G.T.

W. Rheam, of Woodford College, S. Woodford, who died on September 11, 1904.

J. S. Yeo, late Fellow of St. John's College, Cambridge, and Master at Fettes College.

The Council desire to offer their warmest sympathy in particular to Professor W. H. H. Hudson, one of our Vice-Presidents, the name of whose son appears in the above list. Ronald Hudson, in the few years that were allotted to him, had won a wide reputation by his original work, and had won too the personal affection of many friends by his singularly beautiful and loveable character.

In addition to the usual 6 numbers of the *Gazette*, the Council has published two special reports, one on the teaching of elementary mathematics, and one on advanced school mathematics. The editor of the *Gazette* wishes to express his thanks to many coadjutors, and to ask for papers on more elementary matters than he has been able to obtain hitherto.

Professor Forsyth has now served the customary two years of office, and the Council desire to nominate Mr. G. B. Mathews as his successor.

The retiring members of the Council are Mr. Dyer of Eton and Mr. Roseveare of Harrow. The Council nominate as their successors Mr. Dobbs of Eton and Mr. Hawkins of Haileybury.

The President then called upon Mr. F. W. Hill, the Honorary Treasurer, to present his report for the past year. A copy of the accounts accompanies this *Gazette.*

Mr. A. W. Siddons made a brief report of the work of the Committee on the Teaching of Elementary Mathematics, and proposed that the Committee should be re-appointed, which was agreed to.

The reports having been adopted, the nomination of Mr. G. B. Mathews for the office of President for the years 1905-7, and of Mr. Cecil Hawkins and Mr. F. W. Dobbs as members of the Council, were approved.

Mr. G. B. Mathews then took the chair, and a vote of thanks to the retiring President, Professor Forsyth, was passed unanimously on the motion of Dr. F. S. Macaulay, seconded by Mr. C. S. Jackson.

Mr. E. M. Langley then read a paper on "Models and their Uses." This was illustrated by a large number of models, and was followed and afterwards discussed with much interest. The paper will appear in a later number of the *Gazette.*

The second paper was by **Mr. W. H. Wagstaff** on "The New Geometry."[1]

The introduction of "The New Geometry" has brought into prominence many views both of what ought to be taught and how it ought to be taught; the present is a time of unsettlement, and it is only with time, patience, and temperate discussion that we can hope to arrive at a general agreement as to what is best.

We must, as practical men engaged in school teaching, try to frame not merely an ideal course for the individual boy, but the best workable course for the education of boys generally; it follows that we must take account of various disturbing factors. Of these I now mention two—that examinations are, and probably always will be with us, and that boys are not generally educated entirely at one school; these two facts have a very important bearing on the vexed question, shall we, or shall we not, have a fixed order of teaching Geometry?

If each teaching establishment were entirely unconnected with all others and with the rest of the world, each would have its own order, and no difficulty would arise; as things are, however, difficulties may be expected, and my object is to consider not whether a fixed order is best *in itself*, but whether it is best under the circumstances.

First then:—How is the question affected by the fact that candidates from many schools enter for the same public examinations?

In order to make it possible for all candidates to take such examinations, no matter what order of Geometrical study they had followed, some

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[1] This paper has been partly re-written, in consequence of some passages in the original having been misunderstood.

might advocate the total exclusion of bookwork; the paper would then consist entirely of problems, and the student would avail himself of any data with which he was acquainted. This would get over the difficulty; and as we know, there has been a marked tendency in this direction already, but it is doubtful whether all examining bodies would ever adopt it, and still more doubtful whether it is wise; the crammer would regard the proofs of the Propositions as unworthy of attention; he would teach them the facts and not how the facts were obtained, and this would be a great loss, however much the student might be practised in riders.

If then we dismiss the idea of confining all Geometrical examinations to problems, by what other method can we avoid the 'order' difficulty? what is to be the criterion as to whether a particular result may or may not be assumed in seeking to prove some other result?

It is most interesting to note how the Cambridge Syndicate meet this. They say, "Any proof of a Proposition shall be accepted which appears to the examiners to form part of a systematic treatment of the subject." Nothing more comprehensive could be desired; but may we not fear that it is too comprehensive? would it not be often impossible for the Examiners to tell whether a proof really deserved to be passed or not? I hope Professor Forsyth will give us the benefit of his opinion about this.

A second objection to the lack of a universal sequence has been already mentioned, viz., that boys frequently begin their Geometry at one school, and continue it at another. The undesirability of changing the order of propositions in the early stages of a boy's geometrical training is so obvious that I feel I need not labour the point, and I content myself with calling attention to it.

For these reasons I think we ought to have a definite order, if only we could agree upon it; and I hope that this Association will take the initiative in establishing such an order.

Leaving this question I pass on. Perhaps I shall be expected to say something on the merits and demerits of the New Geometry itself; but so much has been said already that I cannot hope to say anything new, and hesitate to say anything at all.

Teachers seem to like it, and so do boys; of course this is an important, but not necessarily the most important consideration. Is the mental training sacrificed? I think it need not be, if the subject is properly taught, and rigorous demonstration insisted on, but I believe it often is sacrificed by slurring over difficulties. When I say a rigorous demonstration, I mean one that will satisfy a good but a mere mathematician, as distinguished from a philosopher.

The experimental side of the work should not be overdone. We want, no doubt, to get the finger-training and the observation-training as well as the familiarity with geometrical figures which the experimental work gives, but, after all, these for most of us are but bye-products, however important; the real object of the New Geometry is to make Geometry not less, but more, effective as a training for the reason.

The New Geometry would seem to admit more boys than the old into the realm of reason-training by geometrical methods; it lowers the margin of cultivation; by its means we can educate not only the clever and the average, but a large number of those who may be classed as stupid. But is it suitable for all boys, however stupid? I think not. And I should make Proposition 4 of Euclid the test; if a boy cannot by *any* means be brought to understand this Proposition, he had better devote his time to some other study than Geometry, however "New." For such boys, be they many or few, I should be disposed to recommend a little logic.

"Nothing is better than wisdom."

"A crust of bread is better than nothing."

"Therefore a crust of bread is better than wisdom."

Any boy can be got to see that, and to see it in some sort of a way where the fallacy lies; and his brains would be better exercised on this sort of thing than on a geometry which he was incapable of receiving.

I may sum up the results of this paper as follows:

1. That in deductive geometry, a definitely recognised standard of order is expedient.

2. That the Mathematical Association should consider whether any steps can be taken which will bring about the recognition of a standard order.

3. That the New Geometry makes it possible for a larger number of boys than formerly to benefit by the study, while it need not, if properly taught, diminish the mental training afforded.

4. That there are boys who do not profit by the study of any deductive geometry; such boys might learn some logic instead; but some training in practical geometry should be given to all.

Mr. Wagstaff concluded with the remark that it would be more interesting if there could be a stand-up fight between the supporters of the New Geometry and those violently opposed to it, and said that he had received a letter from Mr. Baker of Cheltenham, expressing the view that the experimental stage was being overdone.

Dr. Macaulay said that personally he was dissatisfied with the actual changes in the new books on Geometry. He did not know whether he could recollect the things to which he objected most, but one of them was this: far too much time is devoted to practical work. There is a whole section of practical work at the beginning, which is right: then begins a new kind of Geometry, namely, deductive geometry. But in the new books it was not true deductive geometry, for half of it was practical work. He thought such work ought to form but a small part of a course of deductive geometry: occasionally a practical question could be given, but they should be few and far between. It seemed to him that the books so far produced were without definitions and without axioms. Looking at one book recently, he read in one of the first propositions: "Which is impossible, otherwise two straight lines would enclose a space." That axiom had not been stated anywhere in the book. Such an axiom ought to have been carefully explained beforehand, since more than one meaning may be attached to it, although it is generally assumed to mean that two straight lines cannot intersect in two points. What we really want is an improved "Euclid"; we want Geometry made simpler, we do not want it made harder. He admitted it was almost impossible to produce such a Geometry. They wanted a book which was at least as logical as "Euclid." In the New Geometry, they had not yet got half the logic that was in "Euclid."

Mr. E. M. Langley said that Prof. Lodge had suggested to him that he had been attacked. He did not in the least look on Mr. Wagstaff's paper as an attack on him, for in all that he had said he had laid stress on the importance of logical training. The teacher must pay attention to strict logic. If the training is to be really practical, it must be as logical as possible, for a pupil who only learned an unconnected set of 'practical' devices would break down when he came to apply them. He thought it often did not matter what principle or axiom was assumed, if the deductions from it were rigorously made. Under the new system, we do succeed in getting the boys to reason about things whose nature they understand; under the old, we got them not to reason, but to reproduce—sometimes—the reasons given by Euclid from memory. Memory, however, was not to be so much decried as was sometimes done, and should be trained. A good memory would always be valuable. Let them fancy what it would be to deal with the construction of the icosahedron he had been speaking of, if we had forgotten the propositions it depended on and had to go back to first principles. On the whole, in teaching he adhered to the order of Euclid—he hoped an improved Euclid. He took the liberty of striking out and put-

ting in as he thought fit. He thought the great advantage of the new system was that the teachers were no longer fakirs of the great Alexandrian prophet, but could introduce problems of their own for school treatment with the probability of their being just as likely to be set in the examinations as the old book-work.

Mr. W. J. Dobbs said it was generally assumed that the teaching of Theoretical Geometry associated a long chain of argument in which each proposition forms a link, and for that reason it was supposed necessary to adopt some recognised sequence. Our main premise is wrong. Many propositions can be proved directly from first principles. It would be quite simple if, instead of a recognised sequence, we could agree to look upon a few important geometrical facts as fundamental. Proof from first principles would of course sometimes be cumbersome, and a proof should be acceptable if it depends upon one of these standard propositions about which we may all agree. The methods of proof should depend mainly on the possibility of motion of one plane upon another by rotation or translation, or upon a supposed folding of the plane of the figure. Proof by folding ought to be looked upon as one of the fundamental methods, and should be introduced at quite an early stage. It appeals forcibly to young boys. Indeed, as soon as a child can cut out a pattern from the folded edge of a sheet of paper, he can appreciate the idea of symmetry. It is quite unnecessary to insist on some standard sequence.

Mr. A. W. Siddons spoke as Secretary of the Committee appointed by this Association. He addressed himself, first of all, to the question about standard order. If they were to have the best possible order, they must *not* have a standard order. If they had such a standard order, no one would attempt to improve on it. He quite appreciated the difficulty of having no definite order, but was there not possibly a natural order? Quite recently, four people—teachers of Geometry—sat down to draw up a natural order, and the order drawn up by all the four, independently, was practically the same. The real crux was in the first 32 propositions of Euclid: there the difficulty comes in. The title of Dr. Scott's paper was, "Is the New Geometry worth keeping?" They might add, "Are the teachers worth keeping?" for many of them were drifting off in a direction which some of the people who had to do with the change did not intend. He had taken some recent examination papers. The elementary questions are on Drawing, and the candidates would probably get through on the Drawing and the Propositions. The Riders were much too hard, and would probably not be attempted. So, you want to be good at Drawing and good at Propositions. If we want to encourage Geometry, we must not set too hard deductive questions: the questions must be within the pupils' limits. Teachers were, to some extent, being "choked off" deductions by examinations, for examinations were tending to set too hard deductions. Their Committee had inserted one important clause in their recommendations: "Pupils should do something besides mere book-work." They had asked for encouragement to teach something to develop deductive power. That encouragement was not what it should be. What was the object in teaching Geometry at all? They did not teach Geometry merely for the sake of Geometry. He regarded it as a training in logical reasoning. The great object of all teaching is to teach a boy how to learn. Many boys learn things at school and go to the Universities, and then find they do not know how to teach themselves. Dr. Macaulay had talked about too much practical work. Every new part of a subject is better introduced practically than theoretically, and also the facts are much more impressed on the boy's mind by their practical application, and so the end is gained by having a certain amount of practical work. Practical work is not an end in itself. He regretted that many examinations seem to be setting it up as such.

Mr. F. J. W. Whipple did not think enough stress had been laid upon the

great difficulty there was in having a logical order. Anyone who attempts to teach boys, even boys who have had training in New Geometry, the first few propositions of Euclid, or propositions in the newer books, will see that it is in those few propositions that the difficulties occur. They were trying to make their boys of twelve prove propositions which were far more difficult than those propositions which boys learn later. They really wanted a break in the logical order, so that they could go on, come back afterwards, and then take up those difficult propositions. Take Euclid's eighth proposition. A complete proof was impossible: it would involve at least three previous proofs, and it would be very difficult to combine those three. The proposition is one that most boys are willing to take for granted, or to take as the result of a simple experiment. He held that the experiment would be the right proof in a case like that. The experiment made clear, too, a vast body of other knowledge, and they were able to show the connection between geometrical facts without troubling whether they had really got to first principles. That was the course pursued in every other branch of physical science. They did not attempt to find the first principles, but they tried to reach a knowledge of a great many facts, and, out of the great body of facts, to make a chain and correlate the separate facts. Therefore, it is not merely almost impossible to establish an order, but unwise. You show that one proposition can be deduced from another: you can usually reverse the process. If both propositions are equally well known, it matters not which is included in definition or axiom. Mr. Dobbs had entered a plea that the method of folding should be allowed. It was useful, but also very dangerous. It was difficult to make clear what assumptions were being made. Folding was an excellent experimental method: it was excellent in that it showed that the enunciation was plausible. But as a proof it was dangerous, because the boy, having once made his experiment, found it difficult to see that he had not completely proved all that he had been asked for. So, the possibility of a logical development of the subject was destroyed. But, by folding first, they could show that the proposition was plausible, and then, by using congruent triangles, give a sound proof. Reference had been made to the need for omitting some of the propositions quite at the beginning. There would be an enormous advantage, in that the necessity for introducing those propositions at the end would be emphasised. He had not seen a book on the New Geometry which really took up this question. Does a boy understand what he means by straight lines, by parallel lines? There may be a short note when parallels are first introduced, but there is no attempt at a systematic study of the difficulties of the question towards the end of the course. He had no doubt that many teachers did take some opportunity of doing that part of the work. The course in the text-books should include that part of the subject, and should give the teacher a better opportunity of developing it. He hoped that before the Association decided for the necessity of a logical order it would take due note of the difficulties to which he had drawn attention.

Mr. W. E. Paterson said there was one other point. Even more than a recognised order, recognised definitions were wanted. In the last few years there had appeared many different geometries. Many appeared to be quite logical in themselves. Of course many of the proofs must depend on the definitions. Parallel lines were variously defined and the different proofs have to be made to fit in with the definitions. There ought not to be much difficulty as to which were the best definitions; but of the definitions one met, there were few with which one more often than not had not to find fault. Many were redundant; he did not know whether he was using the correct term—they contained too much. For example, take the definition of a square: the old definition was that a square was a four-sided figure with all its sides equal and all its angles right angles. In the new books a square was often defined as a four-sided figure, having all its sides equal and one of

its angles a right angle. Then they forget they have altered their definitions and make use, in a proof, of the fact that the square has *all* its angles right angles. With regard to Dr. Scott's paper, the title it had was given it simply to provoke discussion.

Mr. W. Fletcher said that no one in his senses would seriously propose that a boy who could not learn geometry should be taught logic instead. As an extreme case, proposition four had been mentioned. If a boy could not learn proposition four, he had better give up Geometry altogether, they were told. Now, there were some things which must be assumed or taken as the outcome of practical experience. The perplexities of the beginnings of subjects can be made endless. To keep a boy occupied at the beginning of Geometry until he has a clear conception of what straightness really is, or what parallelism really is, would be just as sensible as to introduce him to the fundamental difficulties of "What is time?" "What is space?" Most boys know whether a line is straight or not, or they *think* they know, and that is all any of us know of everything that we know: most boys know whether a line is parallel to another or not. The more one tried to explain proposition four to boys, the more perplexed the boys become. If you constructed the triangle step by step, the boy realised that the triangle is made. One had to be thankful for that, and he, for his part, had never yet come across the boy who could not understand that: but he would ask, Why should one not build on this foundation instead of puzzling the boy with abstruse questions as to the soundness of the foundation?

Professor Forsyth wished to thank Mr. Fletcher for his remarks. He confessed that he shared the surprise which Mr. Fletcher had expressed concerning one particular recommendation. It was a great surprise to find any one holding an opinion that, if a boy could not be made to understand the fourth proposition in the first book by a combination of practical illustrations and of reasoning, the boy should be turned on to logic. For such a boy with such a mental state, logic would be nothing less than a soul-destroying machine. In early training, pure logic must be kept in its proper place, which is not a prominent one at that stage: and it is best introduced through a medium in which it can be applied, and the speaker thought Geometry an exceedingly good medium for the purpose.

In regard to practical geometry, he was of opinion that in recent times some examination papers had undoubtedly contained too much of what might be called the practical element. It must be remembered though, that they who had urged the association of some practical element with the teaching of Geometry had had for a long time to fight in the wilderness. Only recently had they been allowed to associate the practical element with the other: and it would not be allowed to develop to an improper extent. The proper place of the practical element in Geometry was in providing the boy with the introduction to Geometry. It is undesirable to give him an elaborate mass of definitions and of axioms concerning things of which he had no knowledge whatever; but it is desirable to give him a comprehensible beginning, and then let him be turned on to some reasoning about the things he did understand from his practical work. He would give them an analogy, though he was aware of the danger of most analogies. It was like the teaching of modern languages. In older days, a boy was made to begin, generally, with the alphabet and with the aridities of grammar. But in order to grasp a language, it is better to know the words of the language first, even if they are put together in a wrong order, and then it is possible to pass to the grammar, the practice of which will teach what is to be done with the words. So, in Geometry, the best way is to begin with the practical part, so that a boy has something to go upon.

A good deal has been said about logic and logical order. But the great object of the recent changes was to get rid of one universal logical order; and if one order is to be constructed and mechanically imposed upon all, he

doubted whether they could get a better order than Euclid's. The efforts of the reformers had been directed against the iron constraint of one single and uniform logical order; an iron constraint which prevented freedom, and which, he thought, owing to its very mechanical uniformity, often hindered a sound apprehension of the subject. And he would add that Euclid is only disestablished, not abolished.

In the course of the discussion an opinion had been expressed that it is desirable to have one uniform complete set of definitions, and that such a set of definitions could easily be constructed by common agreement. As the advocate of definitions spoke, he (Prof. Forsyth) thought of the many definitions of parallel lines, and of the controversies which had been waged thereon, and which in their fierceness and spirit had been not unworthy of those of theologians. He trusted that neither this Association nor any body of teachers would ever attempt to re-institute and re-establish one definite order of teaching in Geometry to be mechanically imposed on all. Teachers now have their freedom; let them value their freedom, even if it brought a little chaos. Some of them had recognised that probably at the beginning there would be times of difficulty, of chaos and of confusion; but they must all recognise that before they could make the amplest use of their freedom, they had had to pass through the valley of trouble.

One other thing, Mr. Wagstaff at the beginning of his paper had quoted the regulation of the University of Cambridge as to a proof that may be accepted by the examiners: the proof was to be one that satisfied the examiners as belonging to a systematic treatment of the subject. Now, what more could be asked for than that the boy shall be able to give a proof that seems to the examiners—qualified men and men of experience—to show a systematic treatment of the subject? Only through systematic treatment will that training of the mind, which was their object in teaching the subject, result. The full system is not yet in working order. Critics say there may be differences of opinion on the part of examiners: one examiner may hold different views from another. That is nothing new. Again, it has been said, examining has been made more difficult for the examiners. But the object of training is not the ease of the examiner; and if an examiner finds his work a little more difficult let him bear his burden cheerfully: he is exercising a useful function in a better system.

Then they had been told that if there be no universally recognised sequence, there will result, instead of teachers trying to give a systematic treatment of the subject, a tendency to train in dodges that make for fudging and other intellectually bad practices. He thought that an unworthy suggestion, and one that, he trusted, would not be accepted by any teacher of mathematics.

He did not think that justice was done in Mr. Wagstaff's statement to the Cambridge scheme: for that statement, in quoting only the kind of proof which would be expected, was seriously incomplete and required to be supplemented. The subject matter had been set out in two schedules—one containing the theoretical propositions to be proved, and the other showing the more important practical problems. Now, in drawing up those schedules very considerable care was exercised, and every proposition was subject to a very considerable challenge; propositions were deliberately put with their converses, so that whatever system had been adopted in a systematic treatment of the subject, a proof of the proposition was not limited to that contained in one particular system. It would only happen, perhaps, that the boy was proving first, in the system adopted for him, a part of the theorem that came later in a proof belonging to some other system.

In conclusion, he *did* consider there had been made possible an improvement in the teaching of Geometry in schools. It must be remembered that the examination, whatever worth it might be, was only an incident in the training, and the training was the matter that was of importance. If a

teacher were looking forward to the examination only, he was giving attention to not the most worthy part of education. Teachers who, for examination purposes, neglected other parts of training, were neglecting the most important part of their teaching.

He apologised for the length at which he had treated the subject; but it was a subject that had occupied much of his attention, particularly in recent years, and his interest in it had not ceased.

Mr. Wagstaff, in closing the discussion, said he had been interested in it, and especially by the remarks of those who had attacked him with considerable vigour. He would not attempt to defend himself. He did not intend the suggestion of logic as an alternative to geometry to be taken so seriously. He would adapt his logic to the class before whom he proposed to place it. He did not think he differed from the gentlemen of the opposite side quite so much as they seemed to imagine. He thanked them all for the attention with which they had listened to his paper.

Mr. A. W. Siddons opened a discussion on the proposed new regulations for the Previous Examination at Cambridge. It might be expected, he said, by some present that his line of argument would be that the time which elapses between the scholarship examination and the date of going into residence at the University was an extremely valuable time for the mathematician and scientist; they could develop quietly without having any definite examination in view; but so long as they had to cram Greek for the "Little go" this time was almost wasted. He admitted that there was some truth in this argument, but he did not feel that the burden was really great for any boy who was capable of getting a scholarship, and he strongly urged that there should be no special relief for the mathematician. The boys who really needed relief were the average boys on the modern sides of our schools, boys who had had a good modern side education but who were not perhaps specially gifted with the power of acquiring classical languages, to whom the cramming of a mere smattering of Greek would be no training whatever and to whom such cramming would only give a profound contempt for the Greek language. After quoting from a recent statement by the Master of Trinity, Mr. Siddons urged all members of the Senate to go up and vote when the day came.

In the discussion on the question of compulsory Greek, **Mr. Heppel,** said the question was generally considered in the first instance as to what was to the advantage of all the growing youth of the country. There were some considerations that came before and besides that. A great many people entertained the idea that the word "University" had to do with the spread of knowledge over a great variety of subjects, and that one ought to be able to obtain instruction in any subject one wished. The word "Universitas" in its Latin legal signification meant simply "Corporation," and it was applied at the present day to those corporations which had to do with education and training. He did not think it a wise thing that every University should think it its duty to go over the whole field of knowledge. He did not want to see Oxford and Cambridge competing with new Universities like Birmingham, and he thought those newer Universities which were springing up, and he was glad to see them increasing, would do better work if they confined themselves to subjects likely to be of use to their members. Oxford and Cambridge had eminently distinguished themselves for Classical and Mathematical training. It was not as if the youth of the country could not get the other things. He had every hope that the universities now springing up would do noble work, and that they will do it more completely by devoting themselves to their special subjects. They would do the scientific work, and the work not connected with the ancient languages. He thought the boys who at school had some Greek taught them, might think it a bore; but he could not think that, when they grew up, they would ever regret it. The training in the language was something that lasted. At the

last occasion when that question of Greek was raised at Cambridge, he himself was astonished at the number of mathematical men who decided for the retention of the old system. They felt that something had been introduced into their minds, something to enlarge their ideas which they could never have had without a study of Greek.

He had been interested to observe that the reader of the paper did not demand any special examination for mathematicians. With that he agreed. If Greek *were* to be compulsory, let it be compulsory all round, and if removed, let it be removed all round. He did not think it would be wise to remove it specially for mathematicians.

Mr. C. A. Rumsey said that it concerned the honour and credit of Cambridge University that the present state of things should be altered; that under the existing arrangements men were able to pass in the Greek required of them in the Little-Go by getting up a little grammar and one or two books with the help of cribs; even then it was only necessary to obtain 33 per cent. of the total marks, and on this performance men were hall-marked by the University as having a knowledge of Greek. It was quite usual for the requisite knowledge to be acquired in six weeks, and he believed the record was thirty hours. The Little-Go was, as a matter of fact, only a part of the general examination for the B.A. degree, from the remainder of which it had been decided to excuse those who obtained honours in a Tripos. Now this examination involved a definite knowledge of Latin and Greek, including the power to do unseen passages, and this was the least which ought to be demanded if any kind of certificate was to be given. He therefore advocated one classical language instead of two, with a standard up to translations of unseens. Unseen passages were required at London Matriculation, which really guaranteed a knowledge on the part of candidates of the languages in which they passed. The only satisfactory solution of the Cambridge question was to allow the choice of one or two classical languages with leave to substitute a modern language for one of them if so desired.

Mr. W. D. Eggar said that if as a visitor he might be allowed to speak, he would like to ask a few questions. Some people said the study of Greek would become extinct if these recommendations were adopted. He wished to know what the opinion was about that: were there strong grounds for saying Greek would *not* become extinct? Then, perhaps, Professor Forsyth would tell him what objection there was to two degrees—that of B.A. and, say, some such degree as B.Sc., though that seemed an unpleasant alternative. Still, it did seem desirable, from some points of view, to give some encouragement to Greek. A great many schoolmasters held that Greek should be begun by boys at 15, and not in the preparatory schools; and it was said that if this course was adopted, and if Cambridge abolished Greek, hardly any boys would take it up at the public schools.

Mr. C. S. Jackson, as a non-resident member of the Senate, urged the settlement of the matter by the people on the spot. He regarded it as a monstrous thing that a man, who had perhaps ceased for twenty years or more to have any concern with educational matters, should take upon himself not only to vote for the retention of Greek, but to agitate and urge others to do so in total disregard of the well-considered opinion of the eminent men who were, so to speak, the managing directors of the University. He protested against it, and thought the least such non-resident members could do was to stay away.

Prof. Hudson was by no means certain that if compulsory Greek were abolished, Greek would be destroyed. The proposal was that either Latin or Greek should be demanded. No dire result would follow—relief would only be given to a small number of students. The main argument in favour of the change was that it would tend to destroy the undue specialism in language which was the bane of the education in this country. This was only a very little move in the opposite direction.

Prof. Forsyth said an appeal had been made to him to answer one or two questions, as he happened to be a member of the Cambridge Syndicate dealing with the matter. He would reserve for other occasions remarks of a general kind, and would address himself to one or two points. Mr. Jackson had stated as his opinion that non-resident members should not come and vote. But the appeal had been made on both sides in the controversy, and they had to work under the constitution of the University: no decision could be made by the University as a body without the consent of the Senate. The sole power rested with the Senate. Most of the questions that came up were, of course, settled readily, and outsiders did not come and vote. An appeal had been made to the Senate on this question, and, as members on the other side would go up and vote, he hoped that those in favour of the proposals would go also.

With regard to the question of the "previous Examination," he did not know whether those present would be surprised to hear that Greek was made compulsory as lately as in 1822. They were not dealing with something that had come down to them from the Middle Ages. The regulations were comparatively recent, and some of them were of opinion that, for grave reasons, the time had come for modifying them.

He would give them two other pieces of history. Down to 1850, every man who wished to be qualified for Honours in Classics was required to have obtained Honours in Mathematics; that is, to have passed the Mathematical Tripos. Classics, then, was the persecuted subject. Macaulay, for example, could not take the Tripos: as he said in a letter to his mother that it was impossible for a man to be interested in the formula for $\tan(a+b)$, which he quotes pathetically and incorrectly; he failed to pass the Mathematical Tripos, and took only a poll degree.

Again, down to 1871, only those were eligible for the Chancellor's Classical Medals who had not merely passed the Mathematical Tripos, but had attained the standard of a second class.

The proper changes were adopted, and the exaction of Mathematics was no longer made from Classical students. But he was not aware that, when the changes were suggested, mathematicians arose in the might of their numbers and declared: "This change must not be made; if it is, the study of mathematics in twenty years' time will be extinguished in England": or that they made any similar cheerful prophecies. And so he did not think people were justified in saying that, because this miserable minimum of Greek which is imposed in the "Little-go" would be allowed to be optional, therefore the subject would die out. The subject of Greek had a strong hold in the country. Now, it was four or five times as flourishing as it was fifty years ago. The teachers are more in number; the scholarships and fellowships in the ancient Universities are still open. It could not be damaged by the non-requirement of this miserable modicum, which any fairly intelligent boy could get up in a term.

It was a strange attitude of mind to think that Greek would become extinct, because a number of boys would not take the Greek that is in the "Little-go." He acknowledged that in a large number of the schools in the country Greek is already extinct; but that was owing to the action of the Charity Commissioners, who had drawn up new schemes for the schools, omitting Greek from those schemes. He thought, but was not quite certain, that in cases where Greek was omitted from the new scheme for a school, it had not been taught there at all before the new scheme was imposed. Even so, is this fact to be altered in the least and is the study of Greek to be secured by the conditions of the Previous Examination? It is impossible to believe that the conditions about Greek would affect these schools at all. If the Universities retain these regulations, either boys are to be cut off from the Universities or they get up the subject—from the point of view of educational training? No;—from the object of gaining a knowledge of

Greek life, thought, and literature? No; but from the point of view of satisfying examiners with a miserable modicum of Greek that, in many cases, gives no education and implies no knowledge worthy of the name. In the proposals of the Syndicate, Greek is obtained and still could be taken by those who had something more than a scrap of knowledge; but it would not be imposed upon all, and, having regard to the schools up and down the country, he contended that the changes were by way of furthering the improvement of education in the country.

Sometimes he was challenged as to whether he knew of Greek having done any harm. He could not say he knew of cases where it had done much harm; but he did know of cases where it had done some harm, and many in which it had done no good. He did know of cases where boys of ability had got up this Greek in a single term, being diverted from their special studies, and he had heard, time after time, of young men who regretted—even resented—having been compelled to get up a bit of this language for such an examination as the Little-go was. It had not provided training or knowledge. It was a test that was simply a survival; and it had come to be an exceedingly low test.

He had heard of boys who had spent many years at Greek and never been able to get hold of it, or anything of Greek; they did not learn the humanities, they never grasped the spirit of Greek tragedy; they did not get to know anything of the spirit of Greek literature: those boys were very frequent. Not merely did they not get their minds trained, but they came up to the University without any appreciaticn of Greek literature, sometimes animated by a positive resentment against it, sometimes even with a repulsion to all things intellectual. He trusted all who wished to do something to make things better would go to Cambridge and support the proposals of the Syndicate.

Mr. Eggar had asked: "Why not two degrees—B.A. for people who take Greek, B.Sc. for people who don't?" Well, there was something to be said for it, but the matter was not so simple as it looked at first sight; and further consideration would reveal difficulties and objections about the suggestion when it is made in reference to present Cambridge conditions. They might have a man who was in the First Class of a Tripos who had not taken Greek in the Little-go, and another in the Third Class who had taken the Little-go Greek. Was the first to get the degree of B.Sc. and the other the degree of B.A.? Such a result would not be just. Then it had been suggested that they might institute the degree of B.Sc. for those who know no Greek, but do the ordinary course, and that of B. of Letters for those who know no Science. But the same difficulty came up again when they came to look into the details. Another suggestion was that it might be possible to institute new degrees—B.Sc. mainly scientific, B. Letters for a course mainly literary, and B.A. for a course that involved both Science and Letters.

Of course it would be possible to devise schemes for new degrees that were not dominated solely by the presence or absence of Little-go Greek. But he wished to point out that, if new degrees were instituted which did not involve a knowledge of this minute amount of Greek, then for all who took such degrees Greek would have become optional merely by another way; and those who feared disaster to Greek would presumably be opposed to this project also. Moreover, he could not help noticing that the suggestion of a new degree never was made except when the question of Greek is raised, and he submitted that it was not for those who did not ask for new degrees, but did ask for something else, to construct schemes for those new degrees.

What they of the Syndicate were asking for was a relaxation in favour of education in general, and not for a particular class of people. The language to be taken, if Greek be dropped, must be a modern language, which must be taken up to translation, without the help of a dictionary. The Syndicate

had considered the main reasons which ought to underlie the general scheme of culture and education, and they were asking for the changes on behalf of the great bulk of young men who went up to the University. The speaker concluded by asking all those who agreed with the proposals of the Syndicate to go to Cambridge and vote for them when the day for voting should have come.

THE DEVELOPMENT OF GEOMETRICAL METHODS.

XI.

HERE, again, in this infinitesimal branch of Geometry we find the two tendencies which I pointed out in connection with the geometry of finite quantities. Some, among whom we must place J. Bertrand and O. Bonnet, endeavour to construct an autonomous method directly based on the use of infinitely small quantities. Bertrand's great *Traité de Calcul différentiel* contains several chapters on the theory of curves and surfaces which, in a measure, illustrate this conception. Others follow the usual analytical paths, and content themselves with clearly recognising and throwing into relief the elements which should figure in the foreground. This is what Lamé did when he introduced his theory of *differential parameters.* Beltrami followed this lead when he extended with great ingenuity the use of these differential invariants to the case of two independent variables, *i.e.*, to the study of surfaces.

Just now there seems to be a return to the mixed method, the origin of which is to be found in the works of Ribaucour under the name of *perimorphie.* The rectangular axes of Analytical Geometry are retained, but they are moveable, and connected with the system under discussion in whatever appears to be the most convenient manner. In this way most of the objections that can be levelled at the method of co-ordinates disappear. Thus are united the advantages of what is sometimes called *Intrinsic* Geometry to those which result from the use of regular analysis. But, this analysis is by no means abandoned. The complicated calculations which it almost always involves in its application to the discussion of surfaces and of rectilinear co-ordinates disappear in most cases if we utilise the conceptions of the invariants and co-variants of the quadratic differential functions which we owe to the researches of Lipschitz and Christoffel, inspired by the work of Riemann in non-Euclidean Geometry.

XII.

The results of such investigations are almost apparent at once. The notion that Gauss already possessed of geodesic curvature, but which he had not published, was given by Bonnet and Liouville; the theory of surfaces of which the radii of curvature

are functions the one of the other, inaugurated in Germany by two propositions, which are worthy of a place in the pages of Gauss's memoir, has been enriched by Ribaucour, Halphen, Sophus Lie, and by others with many theories. Some relate to these surfaces regarded from the general point of view. Others are applied to the particular cases in which the relation between the radii of curvature takes a peculiarly simple form—to minimal surfaces, for instance, and also to surfaces of constant positive or negative curvature.

Minimal surfaces have been the object of investigations which form the most attractive chapter in infinitesimal geometry. The integration of their partial differential equations constitutes one of the finest discoveries of Monge; but, in consequence of the imperfect condition of the theory of imaginaries, the great geometer was unable to deduce from his formulæ any method of the generation of these surfaces, nor even of any particular surface. I shall not here return to the detailed historical sketch which I have already given in my *Leçons sur la Théorie des Surfaces;* but it is convenient to recall Bonnet's fundamental investigations, which have given us, in particular, the idea of *surfaces associated with a given surface;* Weierstrass's formulæ, which established the closest relationship between minimal surfaces and the functions of a complex variable; Lie's researches, in which he showed that Monge's own formulæ may now serve as a foundation for a fruitful survey of minimal surfaces. By seeking to determine the minimal surfaces of very small classes or degrees we are led to the conception of the double minimal surfaces which we obtain in the *Analysis Situs.*

In this theory three problems of unequal importance have been considered. The first, relating to the determination of minimal surfaces inscribed in a given contour in a developable which is also given, has been solved by well-known formulæ, which have led to a large number of theorems. For example, every line traced on such a surface is an axis of symmetry.

The second problem, due to Sophus Lie, relates to the determination of all the algebraical minimal surfaces inscribed in an algebraical developable, when the curve of contact is not given. It has also been completely solved.

The third and most difficult is that which physicists solve by experiment, plunging a closed contour into a solution of glycerine. They have then to determine the minimal surface passing through a given contour.

The solution of this problem is clearly beyond the resources of geometry. Thanks to the resources of the most advanced analysis it has been solved for particular contours in Riemann's celebrated Memoir, and in the profound researches which followed or accompanied it. As for the most general contour, its

discussion has been brilliantly commenced, and will be continued by our successors.

Next to minimal surfaces, surfaces of constant curvature were certain to attract the attention of geometers. An ingenious remark of Bonnet's connects both the surfaces, in which one or the other of the two curvatures, the mean or total curvature, is constant. Bour had asserted that the partial differential equation of surfaces of constant curvature could be completely integrated, but this does not appear to be the case. It even appears more than doubtful, if we refer to a discussion in which Sophus Lie tried in vain to apply a general method of integration of partial differential equations to the particular equation of surfaces of constant curvature. But, if it is impossible to determine in finite terms all these surfaces, at least some can be obtained characterised by special properties, such as that of having plane or spherical lines of curvature; and, by the use of a method which has succeeded in many other problems, it has been shown that from every surface of constant curvature can be derived an infinite number of other surfaces of the same nature, by clearly defined operations which only require the calculation of areas.

The theory of the deformation of surfaces in Gauss's sense has also been enriched. To Minding and Bour we owe the detailed study of the special deformation of ruled surfaces which leaves the generators rectilinear. If we could not, as I have just said, determine surfaces applicable to the sphere, at any rate we have attacked with some success other surfaces of the second degree, and, in particular, the paraboloid of revolution. The systematic study of the deformation of general surfaces of the second degree is already commenced. It is among the researches which in the near future will give very important results.

The theory of infinitely small deformation is now one of the most complete chapters in Geometry. It is the preliminary and slightly extended application of a general method which seems to have a great future before it.

Given a system of differential equations, or of partial differential equations suitable for the determination of a certain number of unknowns, it is convenient to associate with it a system of equations which I have called an *auxiliary system*, and which determines the systems of solutions infinitely near to any given system whatever of solutions. The auxiliary system being necessarily linear, its use in all researches throws valuable light on the properties of the system proposed, and on the possibility of obtaining its integration.

The theory of lines of curvature and asymptotic lines has been notably extended. Not only have these two series of lines been determined for particular surfaces, such as Lamé's tetra-

hedral surfaces, but also by developing Moutard's results relative to a particular class of linear partial differential equations of the second order, we have been able to generalise all that had been obtained for surfaces with plane or spherical lines of curvature, by completely determining all the classes of surfaces for which the problem of *spherical representation* can be solved. In the same way, the correlative problem relating to asymptotic lines has been solved by recognising all the surfaces of which the infinitely small deformation can be determined in finite terms. Here there is a wide field of research in which exploration has hardly begun.

The infinitesimal study of rectilinear congruences, already begun long since by Dupin, Bertrand, Hamilton, and Kummer has intervened in all these researches. Ribaucour, who took a preponderating share in these investigations, studied particular classes of rectilinear congruences, and, in particular, those which are called *isotropic*, which intervene in the happiest manner in the study of minimal surfaces.

The triply orthogonal systems employed by Lamé in Mathematical Physics have become the object of systematic research. Cayley was the first to form the partial differential equation of the third order on which the general solution of this problem had been made to depend. The system of confocal surfaces of the second degree has been generalised, and has given birth to the theory of general *cyclides*, in which we may employ the resources of Metrical Geometry, Projective Geometry, and of Infinitesimal Geometry. Many other orthogonal systems have also been discovered. Among them it is worth while to refer to the *cyclical* system of Ribaucour, in which one of the three families has circles as its orthogonal trajectories, and the more general systems for which these orthogonal trajectories are simply plane curves. The systematic use of imaginaries, which we must be extremely careful not to exclude from Geometry, has enabled us to connect all these determinations with the study of the finite deformation of a particular surface.

Among the methods which have enabled us to establish all these results, we should note the systematic use of linear partial differential equations of the second order, and the systems formed of such equations. The most recent researches show that their use will revolutionise most of the theories.

Infinitesimal Geometry was certain to affect the study of the two fundamental problems of the calculus of variations.

The problem of the shortest path on a surface was treated in the masterly papers of Jacobi and Ossian Bonnet. The study of geodesic lines has been followed up, and we have learned how to determine them for new surfaces. The theory of aggregates has intervened and has enabled us to follow these lines in their

course on a given surface. The solution of a problem relating to the representation of two surfaces, one on the other, has greatly increased the interest of the discoveries of Jacobi and Liouville relative to a particular class of surfaces of which we can determine the geodesic lines. The results in this particular case led to the examination of a new question—the discovery of all the problems in the calculus of variations, the solution of which is given by curves satisfying a given differential equation.

Finally, the methods of Jacobi have been extended to space of three dimensions, and have been applied to the solution of a question which presented the greatest difficulties—the study of the minimum properties of a minimal surface passing through a given contour. *(To be continued.)*

MATHEMATICAL NOTE.

164. [K. 21. 1.] *The Constructions in Halsted's Rational Geometry.*

There are some very curious resemblances in the paper by F. S. Macaulay, in the October *Gazette*, to a book which appeared last summer, and these resemblances are scarcely lessened by the perpendiculars used, after the first problem, (1), identical with § 164, being supposed drawn in the *Gazette* without, in the book with, use of parallels, especially as in the problems, *e.g.* (6), identical with § 163, no mention of this is made.

Not only the constructions, but more noteworthy still, the instruments are identical, for the *Gazette* uses the ruler and sect-carrier of the book, though not calling it by Halsted's name, since in fact Dr. Macaulay's article was in type for the July number of the *Mathematical Gazette* and was crowded out till October number, while the book reached the author at the earliest in late September.

I have since learned that the fundamental construction, (1) ≡ § 164, was given by R. F. Muirhead in a "Substitute for Euclid's Third Postulate" (Proc. *Edinburgh Math. Soc.* 1880).

In (2) the *Gazette* solves by using an angle-bisector, a ruler and a sect-carrier, the problem : To draw a perpendicular to a given straight. This it then applies to (3), or by means of a set-square, an angle-bisector, a ruler and a sect-carrier it solves the problem : To draw a perpendicular to a given straight from a given point on it.

Had the book so wished, it could have solved both problems together, not only without parallels, but without set-square and without sect-carrier, with only an acute-angle-bisector and a ruler.

GAMBIER, OHIO, U.S.A. GEORGE BRUCE HALSTED.

REVIEWS.

The Dynamics of Particles and of Rigid, Elastic, and Fluid Bodies; being Lectures on Mathematical Physics. By ARTHUR GORDON WEBSTER, A.B., Ph.D. Professor of Physics, Director of the Physical Laboratory, Clark University, Worcester, Massachusetts. Teubner, 1904. Pp. xi, 588.

The aim of this book, as stated by the author, is to give in a compact form a treatment of so much of the Science of Dynamics as should

be familiar to every serious student of Physics. The range covered is a fairly wide one, including, in addition to the subjects indicated by the title, investigations of mathematical results which may be supposed to be beyond the reader's knowledge, such as theorems in curvilinear coordinates, properties of Spherical Harmonics, and the reduction of quadratic forms to sums of squares such as is needed for the establishment of the normal coordinates of a vibrating system. The result is a book which has considerable merits, and which gathers together a great deal of useful theory. Considerable portions of it are, however, not new, but have been reprinted from the author's treatise on Electricity and Magnetism.

From the points of view of most students of Physics, the book is open to the criticism of being overloaded with mathematical analysis. Conciseness is not always an advantage, but it must be right to state things that are really simple as simply as possible. Professor Webster is too apt to envelop them in a maze of symbols. Take the case of Lagrange's equations. The investigation of them is unduly long, and the case in which the kinetic energy is a homogeneous function of the velocities is dealt with without a clear statement of the condition under which this is true; and, finally, the removal of this condition, though the point which it involves is a very simple one, occupies a page and a half of rather repulsive appearance. To take another case. The component velocities of a point in terms of coordinates referred to moving axes are found by means of the whole array of the direction cosines of these axes and the relations between them; a lengthy procedure, though it may be regarded as a neat piece of mathematics, if that is what interests us. In the course of the work we find that the point is supposed to be fixed to the moving axes, which is not specifically mentioned except in a subsequent article. Eventually the usual expressions in terms of angular velocities are obtained. But the fact that these are almost obvious, and that nobody whose sole object was to obtain them would think of adopting such an intricate process, is not alluded to. In some similar cases the existence of a possible short cut is mentioned, but more often this is left for the reader to discover, if he can, as a pleasing surprise, when he realizes with sufficient clearness the meaning of the result obtained. The investigation of the motion of the centre of mass of a system is a curious example of the author's worst manner. For the space of two pages, including some mathematical analysis which as it stands is not strictly correct, we are led to suppose that the result is connected with the existence of a force function; and on the third page this restriction is removed. Incidentally we are told that the centre of mass of the solar system is evidently a case of a point moving with constant velocity in a straight line. The references to the solar system are not very happy, for elsewhere we find the remark that Kepler's laws give a complete kinematical statement of the law of gravitation for the planets, and we have to read on for ten pages before we meet with any correction of this view.

A proof which is given of Stokes's theorem by means of variations, said to be derived from Helmholtz's lectures, is a neat piece of mathematics; and if Professor Webster here errs in proving the result

in an indirect and artificial fashion, he does so in good company. Why is it that nobody seems to think that a straightforward integration of the double integral with regard to x and y coordinates, such as the following, is good enough?

Let u, v, w be the components of a vector, u_1, u_2, u_3 the differential coefficients of u with regard to x, y and z, and l, m, n the direction cosines of the normal to a surface of which dS is an element of area; then we start with

$$\iint\{l(w_2 - v_3) + m(u_3 - w_1) + n(v_1 - u_2)\}\,dS.$$

Take the terms involving u, and x and y for the two independent variables for this part of the integral, which accordingly is

$$\iint\left(\frac{m}{n}u_3 - u_2\right)dx\,dy.$$

Now $-\frac{m}{n}$ is $\frac{dz}{dy}$ on the surface, thus this integral is $-\iint\frac{du}{dy}\,dx\,dy$, or $-\int(u'' - u')dx$, that is to say $\int u\,dx$ taken round the boundary in the direction which makes with the normal a screw of the same sign as that which a rotation from x to y makes with the axis of z. Thus the theorem is proved for any portion of surface for which projection on the three coordinate planes does not involve any complication. But applying the theorem to an element of surface, it is clear that we have proved that if the vector u, v, w is a definite one independently of the choice of axes, $w_3 - v_3$, $u_3 - w_1$, $v_1 - u_2$ are components of a vector which has the same property. Thus considering the integrals as a line integral of one vector and surface integral of another without reference to axes, also the fact that both can be added for contiguous portions of a surface, we see that, with the necessary restrictions as to continuity, the theorem applies to any case of a portion of surface which is bounded by a closed curve and has two distinct sides.

Professor Webster has done useful service by setting the example of a free use of the term lamellar, applied to a vector distribution which has a potential. Except for its use to specify a laminated structure of magnetization, the word has not been generally adopted. But no better adjective to describe the thing has ever been proposed, and it illuminates the subject in the same sort of way that the introduction into physics of the word vector did. What is wanted is an adjective, and a neutral one, which suggests the geometrical arrangement and nothing more. Professor Webster also employs the old-established term solenoidal; but lamellar and tubular make a better pair, two Latin words instead of one Latin and one Greek. Besides, the word tube is in general use in the same connection, and solenoidal has been somewhat spoilt to the ear by the use of the term solenoid in practical electricity in a totally different sense. Professor Webster also does well to use the term vector product. Unfortunately, Hamilton's scalar product of two vectors has an inconvenient sign, so the term geometric product is adopted instead with the opposite sign. This serves the purpose though not particularly suggestive. Another good word adopted is slide for *glissement* instead of the confusing word shear;

but there does not seem to be sufficient reason for the innovation of giving it half its usual value, considering how much confusion this may make. The same remark applies with less force to curl. The resuscitation of Lamé's first and second differential parameters is also of doubtful advantage; they are not suggestive names. But any way Professor Webster has a better notion of providing enough suitable names for the things he has to talk about than most writers on the subject. He might, especially in writing for physicists, make more use of the conceptions thus expressed to gain conciseness and vividness of statement, and to avoid cumbrous and unnecessary mathematical work; but it is clear that he does not care for conciseness, and that it is not his plan to take any course which could have the appearance of being a short cut, even though it may be one which has a good right to be regarded as the high road.

His account of Helmholtz's theory of vortex motion is a case in which he puts the reader on the right track at the outset: the point to which he calls attention here being the idea of analysing a vector distribution into two components, one lamellar and the other tubular. But he is surely wrong in attributing this analysis to Helmholtz. It was given in exactly the same form by Stokes nine years before Helmholtz's theory of vortex motion was published; see Stokes's papers, vol. ii, p. 254. Here the expression of the tubular component as a curl, that is to say the discovery of the vector potential, is an essential point; but apart from this, the fact that the analysis is possible may be very easily seen. Suppose any vector distribution to be represented in magnitude and direction by lines, forming a fibrous structure in which lines begin and end as is required by the variations of magnitude. Add to this what is necessary to make the system tubular by splitting up each line, at the point at which it ends, into radii uniformly distributed in all directions, and similarly drawing a system of radii up to the point at which each line begins. The system thus added, taken by itself, is lamellar; accordingly the original system is analysed into the tubular system which has been constructed and a lamellar system consisting of the added part reversed. The case of the analysis of 4π times magnetization into magnetic induction and magnetic force reversed is of course an example of this. But there is an advantage in the matter of clearness in getting the geometrical conceptions separated from their physical applications.

There is much in Professor Webster's book which is commendable for thoroughness. For example, in dealing with the flexure and torsion of prisms, we are plunged at once into St. Venant's theory, and are not allowed a chance of entangling the elastic line with any speculations about cross sections in flexure remaining plane.

The treatment of rotating bodies, including tops, and the author's own contributions to the experimental verification of such motions, deserve special attention. W. H. MACAULAY.

Eléments de la Théorie des Groupes Abstraits. Par J. A. DE SÉGUIER. (Paris, Gauthier-Villars), 1904.

This is the short but full first part of a contemplated work of which the master-title is "Théorie des Groupes Finis." The whole will

clearly aim at completeness, so far as present knowledge goes. The present part is difficult reading because of extreme compression, and also because the order of arrangement has been decided by considerations of abstract thought rather than by naturalness of discovery. A principle usually adopted by French authors is that a work worth making public at all is worth writing in sufficiently expanded form, and printing with sufficient prodigality of space, to make it attractive and convincing to those to whom its ideas are new. Lucidity is put on a par with exactness, and to be lucid without being wearisome is to effect a triumph of art. On our side of the channel views as to artistic expression have too often been different. Not to use a word too many, not to waste two lines over an analytic expression which inventiveness in notation can confine to one, have been aims with which we are too familiar. We thus deter scores from gathering anything from what we offer, and comfort ourselves with the belief that the very few who do spend a considerable fraction of a life-time in ascertaining what we mean must really thereby master the subject dealt with, because practically forced to investigate the full content of every sentence for themselves. M. de Séguier seems to have discarded as vanity the pride of his compatriots, to have adopted our false canon of art, and even to have succeeded in outdoing us.

The word "Finis" in the master-title appears redundant. It hardly prepares us to find the preliminary chapter in the introductory volume devoted to infinite assemblages: to an arithmetic in which the two cardinal numbers having prominence are ∞ and ∞^{∞}. The finite is to be studied as a special variety of what is generally transfinite. The once intelligible word "Eléments" also does service in a sense even less elementary than has become usual. We have grown accustomed to a lax use of the term which we associate with diffidence on the part of authors. A comprehensive treatise is often quite unnecessarily, and even misleadingly, described on its title page as one on the elements of a subject, because it starts from the beginning, and the author is aware (as all must be who know what they are talking about) that he has not said the last word possible. But here the word marks no condescension to the beginner at all. Easily grasped ideas are crude, and savour of the concrete. The view has been adopted that it is unscientific to disclose how these have been analysed, and thought has passed back from them to more abstract ones which they involve. On the contrary, fix upon the utmost thinkable which has been passed back to, do your best to reduce it in full generality to the stateable, and call the result an elementary idea. Reason from it as you can, avoiding all acknowledgement of concrete fetters as means of holding it, and so develop what you may call the elements of a theory. Presently descend to things concrete, to the simpler ideas which will help the ordinary human mind in its efforts to grasp the "elementary" conceptions which have preceded. The descent is to be made in the next volume under the heading "Compléments."

The book is to be admired as consistent and masterly. The above remarks are not intended as adverse criticism—many will regard them as amounting to high praise—but as allegations in support of the

present reviewer's opinion that had the author been less careful to avoid diffuseness, and less determined in his adherence to a lofty ideal of rational arrangement, he might have appealed to a wider circle of readers, and have supplied a want of which many are conscious, while still serving higher purposes. All who have thought about groups and aggregates, and want to possess in compact compendium what they know and should know, will welcome the present volume and desire the early appearance of the "Compléments." We share their feelings of welcome and expectation; but at the same time feel more strongly than ever that there is urgent need of a popularizing introduction to abstract group theory, a book which will aim in the first place at awakening, and only secondarily at satisfying, enthusiasm for the subject.

E. B. ELLIOTT.

Die Lehre von der Zentralprojektion im Vierdimensionalen Raume. By Dr. H. DE VRIES. Pp. 78. (Leipzig, Göschen.)

The straight lines which join any point O to all points of a plane trace out a space of three dimensions, and, conversely, the properties of the space may be studied by means of its projection from O on the plane. Similarly, we may project a space of four dimensions from O on to a three-dimensional space and obtain its properties, though the process is naturally more complicated. The author works out this idea in detail, starting with Schoute's "Mehrdimensionale Geometrie" and Fiedler's "Darstellende Geometrie" as his basis. The first part of the book is occupied with a general discussion of the projection, the vanishing points of lines, etc.; then comes the theory of perpendicularity and a series of fourteen fundamental constructions, *e.g.* the line of intersection of a plane and a space, the distance of two points, and so on.

H. H.

Mathematische Einfuhrung in die Electronentheorie. By A. H. BUCHERER, University of Bonn. (Teubner.)

The idea that the phenomena of electrolysis are produced by the transference of small portions of matter carrying an electric charge, is familiar to every student of physics. The discovery that in part of the phenomena of the vacuum discharge we are concerned with the motion of charged portions of matter which have a smaller mass than the ordinary chemical atom, has led to a profound change in our ideas of the constitution of matter. The attention of mathematicians has thus been strongly directed to the study of the motion of small charged systems in accordance with the electromagnetic equations of Maxwell and Hertz, for the question has an important bearing on spectrum analysis and radio-activity.

The problem is one of great difficulty and has not yet been solved in general. At present it must be regarded as in a tentative stage, and thus any book on the subject with any pretensions to accuracy, must involve a constant appeal to experiment.

Dr. Bucherer has been very happy in his treatment of the subject. By using vector notation he has reduced the mathematical symbolism considerably and has thus been able to present the results of mathe-

matical investigation and experimental research in an exceedingly compact manner.

The student will look in vain for a definition of an electron. The simplest idea is that of Larmor, in which the electron is regarded as a charged point. Difficulties, however, arise unless we ascribe to the electron a definite size, and this introduction again raises difficulties as to how the charge is arranged, and what constraints are to be applied. Such difficulties may well be avoided in a first study, and the book can be confidently recommended, not only to students, but also to experimental investigators who desire a compact account of the results of mathematical analysis. G. W. WALKER.

ERRATUM.

P. 111 for "in the Mathematical Tripos this year" read "p. 467 *Nouvelles Annales*, 1903."

COLUMN FOR "QUERIES," "SALE AND EXCHANGE," "WANTED," ETC.

(1) **For Sale.**

The Analyst. A Monthly Journal of Pure and Applied Mathematics. Jan. 1874 to Nov. 1882. Vols. I.-IX. Edited and published by E. HENDRICKS, M.A., Des Moines, U.S.A.

[With Vols. V.-IX. are bound the numbers of Vol. I. of *The Mathematical Visitor.* 1879-1881. Edited by ARTEMAS MARTIN, M.A. (Erie, Pa.)]

The Mathematical Monthly. Vols. I.-III. 1859-1861 (interrupted by the Civil War, and not resumed). Edited by J. D. RUNKLE, A.M.

Proceedings of the London Mathematical Society. First series, complete. Vols. 1-35. Bound in 27 vols. Half calf. £25.

Cayley's Mathematical Works. Complete, equal to new, £10. Apply, Professor of Mathematics, University College, Bangor.

The Mathematical Gazette. Nos. 7-18 inclusive, £1. No. 8 is out of print and extremely scarce.

(2) **Wanted.**

The Messenger of Mathematics. Vols. 24, 25.

Tortolini's Annali. Vol. I. (1850), or any of the first eight parts of the volume.

Carr's Synopsis of Results in Elementary Mathematics. Will give in exchange: Whewell's *History* (3 vols.) and *Philosophy of the Inductive Sciences* (2 vols.), and Boole's *Differential Equations* (1859).

CORRESPONDENCE.

ELEMENTARY PURE GEOMETRY WITH MENSURATION.

E. BUDDEN.

To the Editor of the "Mathematical Gazette."

DEAR SIR,—The non-rigorous treatment of the perpendicular or right bisector and other loci, to which Prof. Lodge refers in his review of my book in the last number of the *Gazette*, occurs only in the experimental introduction. This is meant to be taken orally with the beginner, and is designed to familiarise him with the use of the instruments and with the figures most commonly required in formal and constructive geometry; it is not pretended that the results given in this introduction are formally proved.

In the formal geometry, which follows the introduction, the right bisector and other necessary loci are given quite rigorously; in each case it is shown (i) that every point of the locus is on a certain line or curve, (ii) that each point of this curve is on the locus.—Yours faithfully, E. BUDDEN.

OBITUARY NOTICE.

M. PAUL TANNERY.

WE regret to learn of the death of M. Paul Tannery on November 27th ult., at the age of 61. M. Tannery, after studying in the Polytechnique at Paris, devoted himself professionally to the tobacco industry, and became director of the Pantin tobacco works. He was a man of varied interests, his writings dealing with mathematics, philosophy, the history of sciences, philology and Hellenic studies. In addition to papers on these subjects, he wrote treatises on Greek geometry and the history of astronomy, and collaborated in editing the works of Fermat and Descartes. He once gave a course of lectures on the history of Arithmetic at the Sorbonne, and also lectured on Greek and Latin temporarily in the Collège de France. Recently he has taken an active part in drawing public attention towards encouraging the study of the history of the exact sciences.

BOOKS, ETC., RECEIVED.

The Schoolmasters' Year Book and Directory. Pp. 558. 1905. (Swan, Sonnenschein.)

Lectures on the Calculus of Variations. By OSCAR BOLZA. Pp. 272. \$4. 1904. (The University, Chicago Press.)

Introduction à la Géométrie générale. By G. LECHALAS. Pp. x, 58. (Gauthier-Villars.)

An Introduction to the Calculus based on Graphical Methods. By G. A. GIBSON. Pp. vii, 225. 3s. 6d. 1904. (Macmillan.)

Elementary Algebra. Part II. By W. M. BAKER and A. A. BOURNE. Teachers' Edition. 3s. Pp. 277-468. 1905. (Bell.)

Lessons in Experimental and Practical Geometry. By H. S. HALL and F. H. STEVENS. Pp. vi, 94, iii. 1s. 6d. 1905. (Macmillan.)

An Introduction to Projective Geometry and its Applications: An Analytic and Synthetic Treatment. By A. EMCH. Pp. vii, 267. \$2.50. 1905. (Wiley; Chapman & Hall.)

Formelsammlung und Repetitorium der Mathematik. By O. Th. Bürklen. Pp. 227. 80 pf. 1905. (Göschen.)

Schattenkonstruktionen. By J. VONDERLINN. Pp. 118. 80 pf. 1905. (Göschen.)

Darstellende Geometrie. Vol. I. *Elemente Ebenflächige Gebilde.* By R. HAUSSNER. Pp. 207. 80 pf. 1905. (Göschen.)

Gazeta Matematica. Edited by I. IONESCU. Jan., 1905.

Le Calcul Simplifié. By MAURICE D'OCAGNE. P. viii, 225. 5 frs. 1905. (Gauthier-Villars.)

The Annals of Mathematics. Edited by OSMOND STONE and Others. Second Series. Vol. 6, No. 2. Jan., 1905. (Longmans, Green.)

The Solutions of Differential Equations of the First Order as Functions of their Initial Values. G. A. BLISS. *On the Conformal Representation of certain Isosceles Triangles upon the Upper Half-Plane.* L. WAYLAND DOWLING. *Remarks on a Proof that a Continuous Function is uniformly continuous.* N. J. LENNES.

The American Journal of Mathematics. Edited by F. MORLEY. Vol. XXVII., No. 1. 1905. (Kegan Paul.)

Some properties of a generalised Hypergeometric Function. F. H. JACKSON. *The Relation between real and complex Groups with respect to their Structure and Continuity.* S. E. SLOCUM. *Determination of all the characteristic Sub-Groups of any Abelian Group.* G. A. MILLER. *Collineations whose characteristic Determinants have linear Elementary Divisors. Application to Quadratic Forms.* A. B. COBLE. *On certain Elliptic Modular Functions of Square Rank.* J. A. MILLER. *Minors of Axi-Symmetric Determinants.* E. J. NANSON. *On the Forms of Sextic Scrolls having a Rectilinear Directrix.* V. SNYDER.

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